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ing of the other theory some years before; and, in spite of Professor Ferrel's letter, it still seems to me that I was right in saying that the convectional theory needs revision in the light of Dr. Hann's results, but by revision I do not mean abandonment.

The incompleteness of the new theory is not a reason for being silent about it. It should be welcomed, if only for the reason that it will cause a healthful revision of previous views. The value of multiple working hypotheses has been so well set before our scientific readers, that nothing more need be said on that point. I will not venture to speak for Professor Ferrel, but I am sure that practically every meteorologist in the country will profit from a serious re-examination of his knowledge of the theory of cyclones in the light of Dr. Hann's researches.

As to the process by which the general circulation of the atmosphere shall produce cyclones and anticyclones, it is not to my mind necessary that this should be worked out completely before the suggestion of it may be profitably made. But it does not seem impossible that the general winds might here and there crowd together, owing to irregularity of flow; that, where crowded together, anticyclones would appear; and that, between the anticyclones, cyclonic whirls might be formed. It would be indeed a satisfaction if I could here answer all the pertinent questions, and give all necessary explanations, about such a problem; but, if we may judge by the treatment that dynamical meteorology has received thus far in this country, there is only one American who can do that. I wish that he might consider the possibilities of some such process arising from the general circulation of the atmosphere as is outlined above, and, after working them out rigorously, state them as clearly as he has explained the general circulation of the atmosphere itself. Whatever truth there is in the convectional theory of cyclones would not be harmed by such an investigation, while whatever truth there may be in the hypothesis of driven cyclones would pretty surely be discovered by it.

There is a corollary to the suggestion made by Dr. Hann, that may be of interest to those who seek for an explanation of our past glacial climates. It is generally recognized, that, if there were an increase in the activity of our winter cyclones, there would be an increase of snowfall as well; and, if this were carried far enough, the accumulation of snow might last over the summer. The increase of cyclonic activity would presumably accompany an increase in the general circulation of the atmosphere, if cyclones in our latitudes are driven by the general winds; and this would appear in that hemisphere whose equatorial and polar contrasts of temperature were strengthened. Such strengthened contrasts might be expected in the hemisphere having its winter in aphelion, and particularly at times of maximum orbital eccentricity. I do not mean to imply that a glacial period might depend on this condition alone; yet it may be one of many whose varying combinations at times produce a glacial climate, as Croll and J. Geikie and many others have shown; but this particular element of the combination does not appear to have been recognized.

W. M. DAVIS.

Harvard College, Cambridge, Mass., Dec. 27.

#### Moisture in Storms.

NEXT to the action of heat in storms, the part that moisture takes in them has been greatly emphasized. The so-called "condensation theory" of storms has had wider acceptance than any other. We may imagine a limited portion of the earth's surface heated up by the sun, and this more or less of a circular shape. There will be induced a tendency to an uprising current of heated air, which will continue so long as the central portion is warmer than the air surrounding it at the same level. This tendency, however, would be quickly brought to rest were it not for the fact that the uprising column has its moisture condensed, which liberates latent heat and causes the column to rise still faster. Here is a most remarkable fact, notwithstanding that the release of this moisture diminishes the total amount in the air, and the latent heat warms up the air, both of which causes would stop precipitation at once; yet we are taught that the force of the storm is increased by this process. There is another serious ob-

jection among many. If rain occurred at the centre of the storm, this theory might be plausible; but since the bulk of the rain in this country occurs three hundred miles to the eastward of the centre, and over only about one-fiftieth part of the area covered by the storm, it requires an enormous stretch of the imagination to grasp the causation of our wide-extended storms through this condensation effect. We may add still another consideration. It is fairly well ascertained that the upper limit of our storms, as shown by pressure and temperature observations at Pike's Peak (14,134 feet), is far above four or five miles, and may extend to the limits of the atmosphere. Now, the bulk of our precipitation is formed within 6,000 feet of the earth's surface: hence it is plain that the condensation of moisture plays a very subordinate part in our wide-extended storms, and has nothing to do with their generation or maintenance.

I do not propose to discuss at this time all the objections to this "condensation theory," which have been repeatedly advanced both in this and other journals, and which have not been answered, but I wish to present a recent most extraordinary abandonment of this theory by Dr. Hann, who stands at the head of the old school on the continent. I quote from a translation, by Professor Blanford of London, of a recent statement by Dr. Hann. Speaking against the condensation theory, he says (*Nature*, Nov. 6, 1890), "These views are such as I have always enunciated (for a long time, indeed, without any apparent result) in opposition to the then prevalent theories of the local origin of barometric minima through the agency of condensing water-vapor (as contended by Mohn, Reye, Loomis, and Blanford). They now begin to make way and prevail. Most clearly is this seen in the case of Loomis, who, in the course of his own persistent study of the behavior of barometric minima and maxima, has been compelled by degrees to give up the 'condensation theory' to which he formerly adhered so strongly, and to ascribe the origin as well as the progressive movement of cyclones to the general circulation of the atmosphere."

The importance of this utterance from such an authority cannot be exaggerated. While I have shown that Dr. Hann has been misled by his study of mountain observations, yet it seems to me this avowal on his part reaches out far beyond that. As I have just shown, the very life and existence of the old theory depend upon condensation of moisture. Now, if Dr. Hann, who must understand this fact most thoroughly, has deliberately set it aside, must we not conclude that it has an inherent weakness in itself to his mind. Those who are familiar with Loomis's work will be surprised to learn that he ever abandoned the condensation theory of storms.

It would seem that this controversy over the condensation theory is rapidly culminating, and the indications point to a speedy downfall of that theory. It is a remarkable fact that all the objections urged against this theory, now these many years, have been studiously ignored; but a few words from a recognized authority, even though based upon a wrong interpretation of facts, seem to make headway very rapidly. Surely Hann, Davis, and Blanford form a most formidable front against this theory, and it is high time its defenders should come to its assistance ere it be too late.

H. A. HAZEN.

Washington, Dec. 13.

["Letters to the Editor" continued on p. 8.]

#### NOTES AND NEWS.

AT a meeting of the Royal Botanic Society on Dec. 13, as we learn from *Nature* of Dec. 18, the secretary answered various questions as to the destructive action of fogs on plants. He said it was most felt by those tropical plants in the society's houses of which the natural habitat was one exposed to sunshine. Plants growing in forests or under tree shade did not so directly feel the want of light; but then, again, a London or town fog not only shaded the plants, but contained smoke, sulphur, and other deleterious agents, which were perhaps as deadly to vegetable vitality as absence of light. Soft, tender-leaved plants, and aquatics, such as the *Victoria regia*, suffered more from fogs than any class of plants he knew.